LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.

"The Conspiracy of Silence."

WILL you allow me a word on "The Great Lesson" by the Duke of Argyll? It is especially what is said about Darwin's coral-island theory in the following lines, to which I wish to refer:—"All the acclamations with which it was received were as the shouts of an ignorant mob. It is well to know that the *plébiscites* of science may be as dangerous and as hollow as those of politics. The overthrow of Darwin's speculation is only beginning to be known. . . . Reluctantly, almost sulkily, and with a grudging silence as far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about."

The terms "ignorant mob," "sulkily," and "grudging silence," as used above, cannot readily be forgotten if forgiven by men of science on this side of the Atlantic any more than by their brethren in England.

I am unable to see anything sulky or silent in the exposition of Mr. Murray's coral-island theory of over three pages in length, which was published and sent to all the scientific world in NATURE, vol. xxii. p. 351; nor in the many articles in the current literature and recent geological text-books that have since appeared. In this country no large text-book of geology has been issued since 1880; but Mr. John Murray's work has been fairly discussed, and, so far as I know, has always been recognized. Here at Williams College, for example, the views of Mr. Murray referred to have been expounded each year in the course in geology since 1880. One may, I take it, differ from the Duke of Argyll in accepting or rejecting, wholly or in part, any theory, without laying himself open to the charges quoted above. Of anything like sulkiness or grudging silence I have yet to see or hear the first evidence. There is indeed a "great lesson" in the article by the Duke of Argyll, but it is hardly the one he intended to give.

SAMUEL F. CLARKE.

Williams College, Williamstown, Mass., December 5.

"DARWIN'S LIFE AND LETTERS" are now public property, and as reference to vol. iii. p. 242, shows—what nearly every scientific man knew—that the late Sir Wyville Thomson was distinctly anti-Darwinian in his views, it follows that the Duke of Argyll's inferences as to his reasons for urging Mr. Murray's withdrawal of the "new coral-reef theory" paper from the Royal Society of Edinburgh is illogical, not to say absurd. In justice to Sir Wyville's memory and in support of Mr. Bonney's surmise (NATURE, November 24, p. 77) I wish to state that, talking with Sir Wyville about "Murray's new theory," I asked what objection he had to its being brought before the public? The answer simply was: he considered that the grounds of the theory had not as yet been sufficiently investigated or sufficiently corroborated, and that therefore any immature, dogmatic publication of it would do less than little service either to science or to the author of the paper.

AN OLD PUPIL OF WYVILLE THOMSON'S.

December 17.

Greenland Glaciers.

I HAVE received a letter from Prof. Steenstrup, of Copenhagen, which gives further interesting information respecting the extraordinarily rapid advance of the Greenland glaciers, and corroborates the opinion I expressed in the paper I recently read before the Geological Society, that the rate of advance during the Glacial period may have been far more rapid than that generally assumed, and that that period should be much shortened. Prof. Steenstrup states:—

"Meantime the difference between the Alpine data and the Greenland data seems to have grown greater and greater. During the now returned biennial expedition to our northernmost boundary of the west coast of Greenland, the leader of the Expedition, the clever naval officer, Mr. Care Ryder, has measured a progress or a flow of the great glaciers = 99 feet per diem or in twenty-four hours during the summer, and = 30–35 feet in twenty-four hours during winter months."

This, no doubt, will interest many of your Alpine readers.

JOSEPH PRESTWICH.

Shoreham, Sevenoaks, December 17.

"The Mammoth and the Flood."

IN the notice which you have given of my book, which you are good enough to say is, apart from its theories, a valuable work of reference, I should have been more gratified if you had devoted a little space either to stating my arguments or to refuting them, instead of indulging in a rhetorical wail over my backsliding from the orthodox ways of uniformity.

The theories for which I am responsible have been accepted by so many men in the first rank in science in both hemispheres that I am naturally anxious to have them seriously and severely discussed, and I think your critic would allow that I have justified my hope that this will be the case by converging upon my inferences an unusual array of facts.

It was assuredly quite time that someone who disbelieves in "authority in science" should raise a strong protest against the extravagant position which the English school of geology has taken up on this question of uniformity, an extravagance of which students in other branches of science are hardly aware.

The head of the Geological Survey in this country, speaking not long ago with all the authority and responsibility which surround a President of the British Association, committed himself to the following statement:—"From the Laurentian epoch down to the present day, all the physical events in the history of the earth *have varied neither in kind nor in intensity from those of which we now have experience.*"

This was not the opinion of an irresponsible and eccentric student, but of the official mouthpiece of English geology, and with one notable exception—namely, Prof. Prestwich—it has remained, so far as I know, without protest or repudiation, while Prof. Prestwich himself has been treated as a heretic for the views he has so courageously and ably maintained.

My book is meant to challenge the doctrine of uniformity as generally held by English geologists, and which as held here is largely repudiated both in America and on the Continent.

In regard to its many arguments, I cannot defend them in a letter, but I can shortly examine the only one to which your critic directs attention, and which happens to be a very crucial one.

This is the explanation of the existence of a series of mammoths buried in the tundras of Siberia, throughout its entire length, with their soft parts intact. This fact, which has been known for a century, compelled Cuvier long ago to adopt a conclusion which I have simply accepted and enlarged. I state it shortly in the following extract from my work:—"The facts compel us to admit that when the mammoth was buried in Siberia the ground was soft and the climate genial, and that immediately afterwards the same ground became frozen, and the same climate became Arctic, and that they have remained so to this day, and this not gradually and in accordance with some slowly continuous astronomical or cosmical changes, but suddenly and *per saltum.*" I also argue that the only way I can explain the existence of a chain of such carcasses buried many feet deep in *continuous beds of gravel and clay* is by the operation of one cause only, and that a flood of water on a large scale.

Your critic, who I can hardly think has read the part of my book dealing with this issue, says that the carcasses are found in ice. The fact is, *they are never found in ice*, as the Russian explorers have so well shown. The reference to ice in the account of the discovery of the famous Adam's mammoth has been shown by Baer to have been altogether misunderstood, and nothing is more clear than that they are found buried deep in hard frozen gravel and clay.

Secondly, he urges a view which was generally held fifty years ago, but which has been completely dissipated by the elaborate researches of the Russian naturalists, especially the geologist Schmidt, and which I quote at length—namely, that the carcasses have in some way been floated down by the Siberian rivers and buried in their warp. As Schmidt shows, the

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Siberian rivers make no deposit, either in winter or summer, which could cover in a mammoth. Nor are the mammoths chiefly found near rivers, but on high ground out of the reach of rivers. When they occur near the rivers, it is generally on the head streams, which could not float such carcasses.

Surely, in criticizing my view of a problem which has been the crux of almost every serious student since the days of Cuvier, your critic might have noticed these now elementary facts. It is not fair to me or to your readers to deal with this difficult question as if it could be settled by a casual reference to causes long ago discarded by such authorities as Brandt and Baer, Schmidt and Schrenck.

I am anxious beyond measure to meet with some criticism that I can reply to, and shall not shrink from the issue being tried by the severest tests.

What I complain of, and others more important than myself share my opinion, is that the only answer forthcoming from uniformitarians to test cases like the one above referred to is, ostrich-like, to put their heads in the sand and to cry out, "Since we are committed to Lyell's theory, it is useless to quote facts against us." This may have done in the fifteenth century, but it will not do now when so many critics are abroad.

May I presume to invite a discussion in your paper on this most interesting question? I cannot forget that it was in your pages I first raised it many years ago.

Bentcliffe, Eccles, December 10. Henry H. Howorth.

In regard to the first part of Mr. Howorth's letter, I must remind him that it was admitted in my review that such a being as an irrational uniformitarian did exist, and was duly smitten in his book.

In regard to the occurrence of mammoth carcasses (not skeletons), I wrote of ice with some hesitation, knowing alleged cases to be open to question, but I mentioned it, because, in my opinion, it would be the most difficult to explain, and the strongest case in favour of Mr. Howorth. Where the carcass is preserved in clay or gravel the difficulty is less. All that seems needed is a flood of rather exceptional character, carrying the dead beast rather far north; then, if this happened at the right season of the year, the body might be buried by other floods before decomposition set in (the temperatures might be always low, though sometimes above 32° F.), and so the body might escape unrotted, until it was finally well entombed. My position was that, though this explanation of the escape of a carcass from destruction, under circumstances not very different from the present, was not easy, the explanation of such a series of catastrophes as Mr. Howorth demanded was much harder. The grounds of this opinion cannot of course be stated in the limits of a letter, nor can I discuss *seriatim* the cases which he cites. So far as my memory serves me (I am writing at a distance from any scientific library) they are not so universally favourable to his view as is stated in his letter.

The remainder of Mr. Howorth's letter is open to the charge which he brings against the review, of being merely rhetorical. *Quis tulerit Gracchos de seditione querentes?*

Your Reviewer.

Centre of Water Pressure.

The following extremely simple construction for the centre of pressure of a homogeneous liquid on a triangular area occupying any position whatever in the liquid has not (I learn from a high authority on hydrodynamics) been hitherto known, and it may be interesting to some of the readers of Nature.

Let a particle be imagined to be placed at each vertex of the triangle, its mass being proportional to the depth of this vertex from the surface of the liquid; let G′ be the centre of gravity of these particles, and let G be the "centre of gravity" of the triangular area. Then P, the centre of pressure, lies on the line G G′ at a distance $\frac{1}{4}$ G G′ from G.

There is another almost equally simple way of expressing this result; and of course it is known that there are other ways, more or less practically unmanageable, of representing the position of this point, P, by means of momental ellipses, &c.

George M. Minchin.

R.I.E. College, Cooper's Hill, December 15.

The Recent Earthquakes in Iceland.

On October 28 last, at 20 minutes past 5 in the morning, two earthquakes occurred at Reykjavík, and reports were soon received as to earthquakes in other districts, especially at Cape Reykjanes. The whole peninsula of Reykjanes is covered with lava streams, and there are many craters and fissures. The extreme point of this peninsula seems in former times to have been the scene of many volcanic eruptions. Tradition tells that long ago the promontory stretched eight miles further to south-west than it does now, and that great earthquakes and volcanic eruptions in the years 1389–90 produced the subsidence of the ancient promontory. The land reached then to Eldey (the Fire Island), or, as the Danes call it, "Melsækken." In historic times ten volcanic eruptions are known to have taken place in the neighbourhood of these rocks.

During the night between October 27 and 28 more than forty shocks were felt at the lighthouse of Cape Reykjanes, nine of the lamps were broken, and the house where the lighthouse keeper lives and a warehouse were damaged. A fissure from south-west to north-east was formed in the rocks 2 yards from the lighthouse; the rocks beneath were cracked in several places, and these cracks go in the same direction as the old fissures associated with volcanic cones. At Eyrarbakki the earthquake was observed at 25 minutes past 5, and proceeded from north-north-west to south-south-east. To north-west the earthquake was felt in Borgar fjord, and as far to the south-east as to Eyjafjöll. This shock was therefore felt over an area of more than 4500 square miles.

A less violent earthquake was felt here in Reykjavík on November 13, at 35 minutes past 9 p.m.

In the year 1882 I published in an Icelandic review, ***Andvari***, a list of questions concerning earthquakes, nearly the same as were published in 1880 by Prof. A. Heim for the Earthquake Commission in Switzerland. A similar list of questions has now been printed in the Icelandic newspapers. The questions will also be printed separately, and sent to Icelandic clergymen and others who probably take interest in this subject.

Reykjavik, November 30. Th. Thoroddsen.

The Canary Islands.

Now that the Canary Islands are rapidly becoming better known as one of the most advantageous health-resorts within easy reach of England, it may be of some interest to mention a few facts concerning diseases in the Archipelago.

The one pre-eminent fact is that the climate seems to modify the virulence of the worst, the most dangerous diseases. Puerperal fever, though rather prevalent, is seldom, I may almost say never, fatal, though I know of cases where the patient has been neglected for several days before medical advice was obtained. Diphtheria is also very prevalent in the large towns, owing to the total absence of the most ordinary sanitary precautions, but it seems always to exist in a mild form. I know of certain families who apparently have it frequently, but this terrible disease seems to be only fatal where the most elementary knowledge of nursing is absent.

Fevers of all kinds are lighter in character. The treatment recommended there by the profession is different from that in vogue in England. For example, it starts by a thorough clearing out of the system by means of somewhat violent purgatives and emetics.

Equable as is the climate by day and night, the natives suffer most from chills, which often end fatally. This, I think, may be in a great measure accounted for by the absence of woollen or silken clothing. Those who visit the Canaries from colder northern latitudes where wool is worn next the skin, and who most wisely continue this habit, do not suffer in this way. It is advisable that every article of clothing worn in the islands be either made of wool or silk. Thus armed, one is almost impregnable to the attacks of any disease of a catarrhal nature. Malaria does not exist. Precautions as to hours of recreation, such as keeping in the house at sundown, are in these islands unnecessary, and one may be out on the hottest day at the hottest hour without fear of sunstroke.

The only disease which in any way can be said to be peculiar to, or prevalent in, the Canary Islands is elephantiasis, which, as your readers well know, does not affect well-nourished inhabitants, and is neither contagious nor infectious.

In Gran Canaria diseases of the stomach and intestines are

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